BOOK REVIEWS

Vic Baker, Book Review Editor

GIANT UNDER THE HILL: A HISTORY OF THE SPINDLETOP OIL DISCOVERY AT BEAUMONT, TEXAS, IN 1901. *Judith Walker Linsley, Ellen Walker Rienstra, and Jo Ann Stiles. 2002. Texas State Historical Association, Austin, TX. 304 p. Hardcover, $29.95.*

About mid-morning on January 10, 1901, the world changed forever when the dream of an AWOL mining engineer from the Austrian Navy, a killer of a deputy sheriff, and three east Texas drillers became a reality on a small hill outside of Beaumont, Texas, with the discovery of unimaginable amounts of oil beneath the sands of Texas. Reality turned out to be even greater than the dream, for at first that one well produced over 70,000 barrels of oil each day, twice as much per day as *all* the wells of Pennsylvania. In fact, just the first six gushers at Spindletop alone produced more oil each day in 1901 than all the rest of the world's production put together. Who were these men and what fates brought them together on this cold day in January? That is the story that Linsley, Rienstra, and Stiles tell in their delightful book about Spindletop.

There are few who do not know the story of the ex-railroad conductor, Edwin Drake, and his good fortune near Titusville, Pennsylvania, in August of 1859, but the real entry of the modern oil industry was made almost fifty years later on a hill in East Texas. Oil seeps in Texas had been known since the days of the Spanish explorers and there were oil wells drilled around Nacogdoches in 1886 which produced about 250 barrels a day. By the end of 1897 the region around Corsicana had almost fifty wells with an annual production of over 65,000 barrels, but still the production was much smaller than that of the eastern states. So oil was known in East Texas before the Spindletop discovery, but never before in such overwhelming quantities. That abundance of oil allowed the real birth of the modern oil industry. There was so much oil available that it inspired inventors, industrialists, and business people to seek ways to use it and to make a profit.

Linsley, Rienstra, and Stiles have produced a chronicle of the Spindletop discovery, and also a short history of the early development of the oil industry itself. But the heart of the book is the story of the people involved with the discovery. Pattillo Higgins was a reformed bully and racist who shot and killed a deputy sheriff who had come to arrest him for terrorizing a black church. He was only a teenager and pleaded self-defense, and the not guilty verdict was a nice eighteenth birthday present. Eventually Higgins drifted into a successful career in real estate, the manufacturing of good quality bricks, and he even became a deacon in the First Baptist Church of Beaumont. Captain Anthony Francis Lucas, a trained engineer, was an ex-navy lieutenant ("Captain" was more courtesy title than real) born on the Dalmatian coast. He came to Texas by way of Austria, Washington, D.C., Michigan, Louisiana, and numerous locations in between. The three local drillers who played such an important part in the discovery were the Hamill brothers, Al and Curt, and Peck Byrd, all from Corsicana. Looming large in the background were the two eastern oil men, James M. Guffey and John H. Galey, operating out of Pittsburgh with financing from Andrew Mellon, a name well known in banking circles. The authors have brought this cast of characters, and many others, to life in this intriguing tale.

The numerous illustrations throughout the book make a nice addition to the narrative, but the quality is mixed, perhaps due to the poor quality of some of the originals. The authors have provided an extensive index, end-note references, and a selected bibliography of sources. *Giant Under the Hill* provides the reader with an in-depth look at not only the development of the Spindletop field, but also a brief review of the early development of the modern oil industry as well. This is a true story that reads like a novel, for only in life would a reformed killer, an ex-navy lieutenant, and three East Texas well drillers, all financed by eastern money, change the world.

[Added Note: An interesting companion to *Giant Under the Hill* is Paul N. Spellman's *Spindletop Boom Days* (College Station, TX: Texas A&M Press, 2001), 0-89096-946-9; hardcover) which is a collection of oral histories of people who were living and working in Beaumont at the time of the Spindletop oil boom. Spellman's book describes life in this boom town as it was lived by the ordinary folks.]

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DE ONTDEKKING VAN DE ONDERGROND: ANDERHALVE EEUW TOEGEPAST GEOWETENSCHAPPELIJK ONDERZOEK IN NEDERLAND *(The Discovery of the Subsurface: One and a Half Centuries of Applied Geoscientific Research in The Netherlands). Patricia E. Fasse. 2002. Geologie van Nederland 6, Nederlands Institut voor Geowetenschappen, TNO Utrecht, The Netherlands. No price given.*

On the authority of The Netherlands Home Office, W. C. H. Staring (1808–1877) completed the geological survey of The Netherlands between 1853 and 1860. His map, published in 28 sheets at the scale of 1:200,000, gives a fair picture of the country's surface geology. In 1903 a temporary government survey was established in order to investigate the subsurface geology. Its main purpose was to see whether the South Limburg coalfield extended further northwards. Both dates, 1853 and 1903, were considered of enough importance to justify the publication of this jubilee volume, unfortunately written in Dutch. Nevertheless, the book deserves a brief review in this journal.

The book's four chapters present a review of The Netherlands government's involvement in geology. Chapter one deals with the history of the 1903 survey of subsurface geology, focused on mineral resources. The survey, named "Rijksopsporing van Delfstoffen," was, for most part, under the direction of W. A. J. M. van Waterschoot van der Gracht, who substantially contributed to its success. The main structural features of the subsurface were discovered, as were major reserves of Carboniferous coal (never exploited), Permian and Triassic evaporates, and traces of petroleum. This early survey is usually considered to be the forerunner of the Netherlands Geological Survey (Rijks Geologische Dienst).

Chapter 2 deals with the history of the Geological Survey, which was established in 1920. Quite rightly, this chapter starts with the origins of the earlier mapping by Staring. In 1833 Staring received a doctoral degree from the University of Leiden with a thesis on the geology of The Netherlands, and he carried out his mapping project between 1853 and 1860. Although Staring and other geologists also stressed the need for a revised edition of this first map, this was never taken up until the 1920 survey. The latter eventually completed a new geological map at the scale of 1:50,000, which was nearly completed in print at

the outbreak of World War II. However, a few sheets of this map were never published.

Preparations for a new map were begun after World War II, when more precision was needed and better surveying techniques were available. In spite of the large number of geologists then available, the new map, also at the scale of 1:50,000, is still only half completed. The history of this later mapping receives only limited attention in the study. Other important topics receiving little or no attention are the map of the North Sea, the cooperative mapping with the British Geological Survey, the Geomorphological Map, and the Atlas of Deeper Geology. Chapters 3 and 4 deal, respectively, with human activities and water, the latter being the oldest and most important natural resource of the country. Chapter 3 devotes extensive attention to investigations of urban development plans and the storage of nuclear wastes.

The author is a historian of science, not a geologist. Consequently, the book is more a documentary history than a description of the growth of ideas.

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SHOEMAKER BY LEVY: THE MAN WHO MADE AN IMPACT. *David H. Levy. 2000. Princeton University Press, Princeton, NJ 08540. 303 p. Softcover, $16.95.*

A really interesting read on a legend of a man, Levy's biography of Eugene Shoemaker is extremely informative. The book is chock full of photographs of Gene with family, at work, and at play from early childhood to just before his untimely, accidental death on July 18, 1997. No details were glossed over in this book, including the numerous people Gene influenced, his hobbies, and his famous temper.

Photographs occur throughout the book, averaging about two to three per chapter, and the text is well balanced and comprehensive, spanning a block of time from three years before Gene Shoemaker's birth to three years after his death. The book begins with a description of the discovery of comet Shoemaker-Levy 9, which crashed into Jupiter in 1994. The comet discovery was the culminating highlight of Shoemaker's contribution to science. The book's author, David Levy, was Gene and Carolyn Shoemaker's friend and collaborator in the comet discovery, and hence is eminently qualified to write this biography. Throughout this authoritative biographic work, Levy's careful writing indicates the respect and affection for Gene that many scientists also hold.

The book is organized as follows: Chapter 1. Of Bonding and Discovery: 1993, tells of the discovery of Shoemaker-Levy 9 and how the author came to know the subject. Chapters 2 through 4 discuss Gene's childhood, early manhood, college years, yearning to explore the Moon, marriage to Carolyn Spellmann (his lifelong love and colleague), and his initial journey to Meteor Crater (the first confirmed impact crater on Earth). The name Meteor had nothing to do with the crater, but instead reflected the meteorites found in the surrounding area. Chapter 5, called A Revolution in Earth, sets the stage for Shoemaker's contribution to planetary sciences by relating the 1950s geologic understanding of uniformitarianism versus catastrophism, through a discussion of the age of the Earth, how ideas emerged about faunal dating of the Earth's strata, and the beginning of the theory of plate tectonics. In Chapter 6, Levy writes about the evolution of our understanding of asteroidal impacts, and how, by initiating lunar mapping, Gene

revived Grove Carl Gilbert's 1893 theory that large asteroidal objects impacted the Moon, thereby forming its craters. Chapters 7 through 9 relate how Shoemaker's search for uranium on the Colorado Plateau and obsession about the Moon led on to his pushing the research of Ed Chao (who discovered the mineral Coesite at Meteor Crater, proving the crater was of impact origin), to inventing the field of Astrogeology by establishing the Astrogeology Team within the U.S. Geological Survey, and to the training of Apollo Astronauts. Additional chapters discuss the numerous NASA missions that Gene led or participated in, Gene and Eleanor Helin's initial proposal to use astronomical observatories to track and monitor the solar system asteroids and comets, Gene's mapping of Australian impact craters, and the observation of Shoemaker-Levy 9's crash into Jupiter. Gene's lasting legacy is enormous, but his most important contribution was to help establish a balance between catastrophism and uniformitarianism in solar system processes. The debate about this balance has been ongoing since the success of the uniformitarian paradigm in the 1800s. Uniformitarianism defined the geoscience horizons of the following century through interpretation of terrestrial geology in terms of strict, non-catastrophic processes. Through Shoemaker's contributions and those of others, this mindset has since given way to an actual view of geologic processes that allows for the occurrence of short-duration events that cause extreme changes in the surface of the Earth. This realization was triggered, in part, by the growing recognition of the importance of impact structures in our solar system's planets and the continued discovery and tracking of asteroids and comets. Levy's final chapter, The Last Voyage, notes how Shoemaker's 1948 dream of going to the Moon was finally realized in 1999 as NASA's Lunar Prospector spacecraft crashed near the Moon's south pole, delivering Gene's ashes.

The book is an excellent source to help understand the evolution of planetary science to which Gene contributed. It goes into details about Gene's reception of numerous awards, his relationship with Lowell Observatory, and about how from 1962 to 1985, Shoemaker blended his astrogeology research for the U.S. Geological Survey with teaching at the California Institute of Technology (Caltech), chairing Caltech's Division of Geological and Planetary Sciences from 1969 to 1972.

One of Gene's gifts was his cheerleader's ability to influence people for the better, being an unfailingly generous and intellectually honest friend and mentor. His colleagues remember an exceptionally brilliant, exuberant, vibrant man and a warm human being, who occasionally indulged in angry antics and whose loud, happy laughter often rang down the hallways of scientific buildings. Levy's book captures Gene's life and personality very well indeed.

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TERTIARY HISTORY OF THE GRAND CAÑON DISTRICT. *Clarence E. Dutton. 2001. (Reprint of the 1882 edition) Introduction by Wallace Stegner. Foreword by Stephen J. Pyne. The University of Arizona Press, Tucson. 368 p. Hardcover, $75.00.*

The original 1882 edition numbered only three thousand copies. The only reprinted edition that included the atlas appeared in 1977. As the premier early description of then-remote canyon country in the southwestern U.S. during the nineteenth century, Clarence E. Dutton's *Tertiary History of the Grand Cañon*

District is now available in the twenty-first century to a wider audience, thanks to its reprinting by The University of Arizona Press. This new edition (without the accompanying atlas) contains the Introduction by Wallace Stegner (1913–1993), reprinted from the 1977 edition. Stegner wrote his doctoral dissertation on Dutton (1841–1912) and also discusses the immeasurable contributions of the book's two illustrators, Thomas Moran (1837–1926) and William Henry Holmes (1846–1933).

What is new in this edition is the Foreword by Stephen J. Pyne, author of *How the Canyon Became Grand* (1998; reviewed in *Earth Sciences History,* 1998, 17:222). The foreword's title, "Dutton's Point," sounds almost like a play on words. The "Point" refers to Point Sublime as viewed from the canyon's North Rim. But Dutton's other "point" in writing this monograph was not only to describe the geology and landscape but also to attempt to convey the majesty of this region—a region shunned by explorers for three centuries. Here was scenery that was an acquired taste. We forget that people of the nineteenth century were raised primarily in humid, green landscapes and did not—indeed could not—immediately appreciate this harsh, arid, red-rock country. *Tertiary History* was instrumental in educating the public toward such appreciation.

Dutton was the first person to attempt to describe the tongue-tying experience of viewing the Grand Canyon (spelled "cañon" throughout). There can be no rapid reading of this text. The author is clearly enraptured by his subject and exhausts the vocabulary of superlatives before the book is half finished. When he starts to discuss the effects of atmospheric influences on the colors seen from Point Sublime, he flatly states: "And here language fails and description becomes impossible" (p. 152) Words like "colossal," and "stupendous magnitude" may sound quaint or over-the-top, depending on one's modern perspective on word usage. But anyone who has actually visited the Grand Canyon knows that Dutton does not exaggerate in the least. (The language is rather analogous to seeing a classic movie where a phrase originated—and became a cliché when less imaginative writers simply copied it.)

The value of *Tertiary History* today is more literary than scientific. But it presents in beautiful language the state of scientific knowledge at the time, which is critical to science historians. A most intriguing passage occurs in the final chapter, "Details of Erosion" (pp. 258–259, including Plate XLII "Rounded inward curves and projecting cusps of the walls"):

> . . . One of the most striking features in the vast maze of cliff-work in the Grand Cañon is found in the extremely tortuous lines of frontage. . . . The first view is extremely confusing, and under the many causes of optical delusion prevailing in the landscape, it is difficult to see anything but chaos—an utter absence of anything like system or arrangement. But patient study and analysis at length reveal many striking evidences of order. If we consider any one of the larger amphitheaters opening laterally into the main chasm, we shall note that it has many lateral amphitheaters opening into it of an inferior order of magnitude. (p. 258)
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> The cusp contour is also repeated on a minor scale in the wall faces, where it appears as a minor decoration or fretting of the edges of the strata. (p. 259)

Dutton was in no way responsible for developing chaos theory or discovering fractals. But I wonder what mathematician Benoit Mandelbrot, the discoverer of fractals in the mid-1970s, would have to say about Plate XLII.

I checked an original copy of this monograph at the U.S. Geological Survey library in Menlo Park, California, and am pleased to report that the reproduction in the new edition of both the color and black-and-white plates is excellent. In the text, a period is occasionally missing or indistinct at the end of a sentence; this precisely reflects the original.

The accompanying atlas was not reprinted with this edition, no doubt due to prohibitive cost. This is no detraction, because the text as literature is more than

capable of standing alone. No matter how many books on the Grand Canyon you may already own, if you are a canyon aficionado, you really should have your own copy of *Tertiary History*. For description of a landscape where, in reality, words fail to convey what the eye sees, Dutton remains the master.

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MIND OVER MAGMA: THE STORY OF IGNEOUS PETROLOGY. *Davis Young. 2003. Princeton University Press, Princeton, NJ. 686 p. Hardcover, $69.95.*

At last, we finally have a comprehensive, up-to-date history of igneous petrology. Davis Young, well known for his earlier book on N. L. Bowen, has put together an outstanding account of petrologic studies and the evolution of theoretical concepts from the earliest classical scholars down to the present day. To my knowledge, this is the first work of its kind since Loewinson-Lessing's *Historical Survey of Petrology,* which appeared fifty years ago.

Young divides the history of igneous petrology into periods that were distinguished by marked conceptual advances resulting mainly from the introduction of new instrumentation, such as the microscope and new analytical tools, or the application of principles derived from other fields, such as fluid mechanics. Not only has he done a masterful job of organizing a staggering amount of material, much of it from obscure sources, but he also presents his results in a remarkably readable form. I confess that I had trouble plowing through four chapters devoted to the classification and nomenclature of igneous rocks, but I was nonetheless impressed with the manner in which Young demonstrates that these periodic efforts to arrange igneous rocks in orderly boxes are not just pedantic exercises but attempts to codify new ideas and apply them to rocks in an orderly way. It is only natural that every major advance in petrology has been closely followed by such a re-assessment of how igneous rocks are defined and genetically related to one another.

Though its scope is very broad, the book is not encyclopedic. As Young states at the outset, it would be impossible to discuss every aspect of petrology over such a long period. Instead, he has made a conscious effort to focus on work that has had a notable influence on our general interpretations of igneous rocks. Thus, certain memorable works are discussed in great detail while others may be mentioned only briefly, if at all. Not everyone will agree with his choices. Little is said about our struggle to reconcile the compositional features of igneous with tectonic processes such as sea-floor spreading and subduction. In this respect, significant studies, such as those of ophiolite complexes or the ocean-drilling program, might deserve more attention. The same is true of certain notable petrologists. I would like to have seen credit given to one of my personal heroes, Ferdinand Fouqué, for his early recognition that most felsic rocks have lower melting temperatures than mafic ones and that the end-product of crystal fractionation is a felsic liquid rich in silica and alkalies. His observation that olivine-bearing and olivine-free basalts have separate lines of descent came long before Kennedy proposed it in 1933. And it was he, rather than Howel Williams, who established the origin of calderas by collapse into evacuated magma chambers. On the other hand, I gained a better appreciation for the remarkable work of several early petrologists whose names were only vaguely familiar to me. Some of these, most notably Bunsen, Durocher, and Sorby,

emerge as outstanding, while others I had held in high esteem come out looking rather shabby.

In following Young's discussions of mechanisms of magmatic differentiation, which, of course, are at the core of igneous petrology, I found it interesting to compare the views he stresses with those of Loewinson-Lessing fifty years ago. The earlier work made a marked distinction between differentiation by fractionation of crystals and that by fractionation of liquids. It dwelt at greater length on the latter, even though two of the principal mechanisms of liquid fractionation, immiscibility and Soret diffusion, had been discredited in the minds of most petrologists. We find that these processes are still with us today. Immiscibility is now recognized as an important factor in a number of important systems, including highly alkaline magmas, carbonatites, sulfides, and very iron-rich tholeiites, such as those of the Skaergaard intrusion. Even Soret diffusion has seen a recent revival. We have also witnessed a general acceptance of convective fractionation of liquids resulting from the contrasting densities produced in gradients of thermal and chemical diffusion near a steep front of crystallization. Young takes due notice of these, but his main focus is on crystal fractionation, particularly crystal settling.

Most readers will find the final chapters to be the most interesting part of the book, because they deal with topics that many of us witnessed or even participated in. The discussion of the dispute between Bruce Marsh and the Cambridge group over the nature of convection in magma chambers is outlined in a very even-handed manner that lets the reader decide for himself where the answer lies. The same is true of his discussion of the principles brought out in the prolonged debate over the role of crystal settling. It is interesting that, even as more problems are heaped on this time-worn mechanism of differentiation, it refuses to die. Similarly, we have recently witnessed a lively debate over whether the petrographic features of coarse-grained rocks can be reliably interpreted in terms of the once-popular "cumulate paradigm." Recent work showing that most coarse-grained rocks have been re-equilibrated during long periods of slow cooling and no longer preserve their original petrographic character simply repeats Cotta's warning in 1858 that "No rock remains completely in the state of its original formation." It seems that some ideas are simply too deeply ingrained to die.

The most lasting impression that Young's story leaves with me is how vital a few innovative individuals have been in stimulating conceptual advances. This is true even when the ideas were misguided. Werner, thanks to his persuasive influence on students and his steadfast refusal to abandon a seemingly logical but erroneous idea, had a more profound effect on petrology than a host of conventional geologists who simply followed the crowd. He inspired others to examine their rocks more closely and gather the solid evidence needed to provide better explanations. Similarly, when Bowen proposed an intellectually appealing system based on the elegant experiments that led him to conclude that granites are products of crystal fractionation of basalt, he caused others to scrutinize plutonic rocks more carefully and eventually discover more satisfactory theories. These and other examples warn us that innovative, thought-provoking work seldom survives the criticism of reviewers who are unable to entertain unconventional ideas. As Hannah Arendt put it, "better a creative error than an uncreative truth."

Davis Young deserves our appreciation for a book that is not only a scholarly review of the history of igneous petrology but also a thought-provoking account of how our science advances.

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CELEBRATING THE LIFE AND TIMES OF HUGH MILLER: SCOTLAND IN THE EARLY 19TH CENTURY, ETHNOGRAPHY & FOLKLORE, GEOLOGY & NATURAL HISTORY, CHURCH & SOCIETY. *Lester Borley, ed. 2003. Cromarty Arts Trust and Elphinstone Institute, 4 Belford Place, Edinburgh EH4 3DH, Scotland. Softcover, L13.50.*

The bicentenary of the birth in remote Cromarty, northwest Scotland, of the writer and geologist Hugh Miller in 1802 was celebrated from almost every possible perspective at meetings held in his place of birth in 2001 and 2002. *Celebrating* represents the published versions of twenty-six papers presented at the second conference, which was attended by some 150 persons. The papers were organized into an opening plenary session, then sessions on ethnography and folklore, geology and natural history, Church and society, and a closing plenary session. The findings of the different sessions were duly summarized, and these summations are also published in the volume. While the sections on folklore, Church politics, etc., were surely of interest to the participants—who were engaged in a fine expression of Scottish nationalism (a very proper activity)—I shall only comment here on the papers dealing with Miller's geological and general scientific work, and James Secord's remarks at the closing plenary session.

The ever-active Hugh Torrens spoke on the stratigraphic methods used by William Smith and their application in economic geology, especially coal prospecting. He has written on this topic extensively in the last few years, and at first I thought I had heard it all before. But not so!

In setting up his stratigraphic column, Smith considered strata that lay above and below the 'Oolite' rocks of the Bath region, where he had commenced his geological investigations. Torrens draws our attention to a passage in Miller's *Old Red Sandstone* where he stated that the Oolite was Miller's "meridian line," from which reference line his "geological scale had been graduated on both sides." Thus the geological autodidact Miller recognised in 1841 the significance of Smith's stratigraphy, how it worked, and how it might have economic significance so far as coal prospecting was concerned, as a way of distinguishing between strata at different stratigraphic horizons.

Torrens further discusses the history of early attempts at coal prospecting in Scotland, pre- and post-Smith, and the problems involved in the extrapolation of English stratigraphy to distant Scottish strata; or the arrival of the 'geological scale' in Miller's part of the world. The attempts at coal prospecting in the Cromarty area are discussed, with evidence from Miller's writings that he recognised that they had been, and were, doomed to failure. This recognition says much about Miller as a Smithian geologist; and seemingly he had become one largely by his own reading as an autodidact, coupled with his personal recognition of the importance of palaeontology in geological investigation. Some years ago (1996) I published a paper on Miller's work as a geologist, which Torrens is kind enough to refer to as a "masterful survey," but I now realise that I did not do the necessary footwork on Smith's geology that Torrens has undertaken, so that the *significance* of Miller's remarks on coal prospecting had escaped me.

My essay on Miller's work on fossil fish is also totally overshadowed by the contributions of the palaeontologists Nigel Trewin and Philippe Janvier of Aberdeen University and the *Muséum d'Histoire Naturelle* (Paris), respectively. Dr Trewin's essay discusses the relationship between Miller's thinking and that of Louis Agassiz respecting "Miller's winged fish," *Pterichthys* (*Pterichthyodes*)

milleri, and he tells the reader many interesting facts about the modern understanding of the animal and its taphonomy.

Janvier's essay, which may properly be described as masterly, discusses in detail Miller's reconstructions of several Devonian fish-types, providing comparisons with modern interpretations. This takes us into the history of issues in fish classification and reveals the sophistication of Miller's drawings and fossil reconstructions. This is not whiggish work. The modern comparisons help understanding of what Miller accomplished. We also learn about the ideas of Miller and Agassiz on the "three-fold parallelism" of the development of embryos, the order of fossils in the stratigraphic record, and the "natural system" of classification, a matter about which Robert Chambers rightly made much, disturbing many Victorian minds in the process.

Janvier brings forward a quotation from Agassiz to the effect that the 'highest' fishes for any geological system appear first in the stratigraphic record. This idea was taken over into Miller's writings, but I had not realised previously that it came straight from Agassiz. However, for Agassiz 'highest' meant 'most general' rather than 'most advanced'. So there was, for him, nothing essentially anti-evolutionary in this particular aspect of his thinking (though he was a fierce opponent of evolutionism). As for Miller, he grasped the idea and gave it a theological twist. He thought humans were 'fallen' or exhibited moral degeneration. Likewise the same tendencies ('Nature's way') were manifested also in the stratigraphic record of Devonian fish! The big ones came first; and then the little ones! For all his erudition and palaeontological expertise, Miller was no modern.

John Hudson provides a first-rate account of Miller's work on the Isle of Eigg in the Inner Hebrides, which topic he has been studying from his days as a research student in the 1960s. His account focuses on Miller's understanding of the pitchstone that forms the highest hill on the island, and overlies the interesting fossil remains of a pine forest. And Miller's work on Jurassic fossils on Eigg, particularly his discovery of a Plesiosaur, is usefully analysed. Miller only found a few scattered remains, but recent study of the site has, we are informed, yielded enough pieces to enable a replica of the animal to be prepared for the National Museums of Scotland. Examining the Oolite fossils of Eigg, Miller also opined that the sandstones there were the products of estuarine deposition.

Simon Knell, who has made extensive studies of the history of collecting in Britain, naturally writes on Miller's work as a collector, discussing the how, where, when (not on Sundays), and the whyfore of his collection practices. He obtained such a quantity of material that he eventually established an annex to his house in Edinburgh to serve as a private museum. Knell argues that Miller's motivation for collecting was not primarily linked to his religious concerns or beliefs but was related rather to his pleasure in outdoor activity, the scientific interest of his finds, and the perception of the social advantages common in his day of having an extensive, well-presented collection. But Miller also came to 'use' his collections for the purpose of metaphysical argument. Importantly, they were purchased after his death and subsequently came to form the chief nucleus of the present collections of the National Museums of Scotland.

The paper by Alison Morrison-Low and R. H. Nuttall, two authorities on the histories of scientific instruments in the nineteenth century, is primarily about just *that,* and finds rather little to say about Miller's microscopes or precisely what he did with them. However, Professor Hudson mentions Miller's sectioning and examination of sharks' teeth found at Eigg. There is also quite a well-known photo-portrait of Miller using a hand-lens (though not in the approved manner with the lens held close to the eye), which is duly reproduced, but the authors

fail to identify the microscope that Miller owned or used and the one on display at the Miller Museum in Cromarty is not, it seems, actually his. Miller did, however, report using a microscope to examine sections of the 'Eigg Pine', apparently being able to show with his instrument what was or was not a conifer.

Michael Collie, who has made extensive studies of the network of amateur naturalists of northwest Scotland in Miller's day, and the relations of their work to that of the 'big-shots' such as Murchison, and the assistance they rendered to such 'central' figures, covers this ground again, appropriately focusing this time on Miller. Though Miller contributed significantly to the supply of material to experts at the centres of learning, Collie regards his chief accomplishment as being that of a geological writer, who, had he lived longer, would presumably have had to have changed his style to accommodate himself to the emerging professionalisation of science. But now that we have the benefit of Janvier's study, one may think that Collie somewhat underestimates Miller's concrete empirical and conceptual accomplishments in palaeontology.

The last paper in the geological section is by Ralph O'Connor, a research fellow at St John's College Cambridge, completing his PhD on "Popular Geological Writing in Early 19th-Century Britain," presumably under the supervision of James Secord, for whom such a topic would be just the ticket. O'Connor discusses the dioramas and panoramas such as were popular in Victorian Britain, and shows how Miller's writings meshed well with these popular 'spectacles'. They provided word-pictures of the strange creatures of the world's past. We tend to take such odd creatures for granted today, but they were something strange, new, and wonderful in Miller's time. Lost sea-monsters, and even Miller's strange 'winged fish' could be nineteenth-century counterparts of the dragons and such of bygone ages. But the newly discovered bones were undoubtedly *real.* And that reality could be displayed by a wordsmith such as Miller almost as well as by museum exhibits.

James Secord rounds off the geological components of the book, and ruminates effectively on the technological and social changes since Miller's day. Besides underscoring the importance of *writing* in Miller's overall contribution, Secord emphasizes that for Miller the burgeoning problems of the emergent technology of the industrial revolution were not due to failures in economics or technology itself. Rather he thought that the future might be bleak because of a failure of faith. Secord refers to the ongoing professionalisation of science in Miller's day, such that Miller himself might not have flourished as a scientist in the modern world. But Secord rightly points out that there is an intercrossing of popular and professional science today just as much as ever there was in the Victorian era, which Miller exemplified and contributed to so well. But Miller's idea that all could be put right by religious faith and observance has, I think, been shown by the modern world to be a grand delusion. But that is a topic for another and different book.

Meanwhile, the contributors have produced a fine collection of papers on Miller's geology, complemented by a useful appendix in the form of a "Hugh Miller Cromarty Trail," by means of which visitors to Cromarty may easily locate all the places in the town and its neighbourhood specifically associated with Miller. His countrymen remember him well, and not just as a heroic Scotsman, but as someone still to be studied and understood. This book, too, will repay study, and will promote understanding of the past and the present—the reciprocal 'keys'.

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CHARLES DARWIN'S *THE LIFE OF ERASMUS DARWIN.* *D. King-Hele, ed. 2003. Cambridge University Press, Cambridge, U.K. 172 p. Hardcover, $25.00.*

Given the scale of the multinational Darwin industry over recent years, it is difficult to conceive that any of Charles Darwin's output of books and articles, or assemblage of letters, notes and manuscripts have eluded publication, re-publication, exegesis, or commentary. The publication of the *Correspondence,* which began in 1985, continues its stately progress, and we have now reached 1865 with volume 13—somewhat less than halfway according to the listing in the *Calendar of the Correspondence.* In this context, Charles Darwin's account of his grandfather, Erasmus, published in 1879, is something of an anomaly, having hitherto escaped the close scrutiny to which the vast majority of his other works have been subject. This new version, edited by the pre-eminent Erasmus Darwin scholar, Desmond King-Hele, is therefore a welcome addition to Darwinian scholarship. The text is unabridged in the sense that the editorial cuts made to the proof copy of Charles's original version by his daughter, Henrietta, have been reinstated. Whether this can, therefore, be regarded as the definitive text is moot, since Henrietta's editing was carried out at Charles instigation and simply repeated an editorial collaboration that had become well established. But as King-Hele uses italics to indicate the reinstated material, we effectively have the benefit of both versions in the single volume.

Erasmus Darwin was a towering figure in the intellectual life of late eighteenth century England and beyond. A founder of the Lunar Society, a group of like-minded inventors, experimenters, manufacturers and thinkers, including Matthew Boulton, Samuel Galton, James Watt, and Josiah Wedgwood, who met monthly in Birmingham in central England, Erasmus seemed to incorporate in a single individual much of the diversity of interests and expertise that this group as a whole represented. Although his primary occupation was as physician, his status in England as a poet was briefly unequalled before fashions changed and his reputation rapidly declined. William Godwin described him as "so extraordinary a man, so truly a phenomenon," while Samuel Taylor Coleridge commented that he "possesses, perhaps, a greater range of knowledge than any other man in Europe, and is the most inventive of philosophical men." He was elected to the Royal Society in 1761, became a fellow of the new Linnean Society in 1792, and was elected to the American Philosophical Society in 1793. He had radical views and came close to being tried for sedition in the fevered political atmosphere of the early 1790s, but on finding himself associated with insurrection, he retreated into science, and his drive for change focused on developing a biological vision of evolution rather than involvement with the politics of the day.

The origins of Charles Darwin's 'biography' of his grandfather are somewhat unusual. It arose initially out of a special *Gratulationsheft* issue of the German evolutionary journal, *Kosmos,* marking Charles's seventieth birthday on 12 February 1879. One of the contributions was an article by Ernst Krause entitled "Erasmus Darwin, the grandfather and forerunner of Charles Darwin." On 9 March Charles wrote to Krause, offering to have the article translated into English. Krause, in turn, offered to revise his essay, and Charles agreed, saying that he would add a 'preface'. Collecting information from various cousins, and then discovering that he already had in his possession in a long-forgotten box of his father's hundreds of letters by Erasmus, Charles began his preface in mid-May. It rapidly expanded to a 'Preliminary Notice' of around one hundred pages, which he completed in early June. Following a rapid turnaround characteristic of nineteenth-century pub-

lishing—but apparently unattainable by academic publishers of the present era—Charles received proofs in July which he circulated amongst various relatives. Daughter Henrietta and son Leonard were critical, and Leonard suggested that Henrietta, who had previously provided editorial assistance to Charles on previous books such as *The Descent of Man,* should cut up and rearrange the proofs so as to reduce the length of the text. Having other projects to pursue, Charles appears to have been happy to leave the organising and editing of the final text to Henrietta. Although this still turned out to be half as long again as the translation of the revised version of Krause's article (which Charles had abridged), it was the latter's name that appeared more prominently on the title page of the first edition which was published by John Murray in November 1879. It was not a particularly popular book, only 800 to 900 copies having been sold by May 1881. In 1887, after Charles's death, his son, Francis, brought out a second edition in which the title page was changed to give primacy to Charles's contribution.

In his introduction, King-Hele draws attention to Henrietta's editorial role, which extended beyond corrections and stylistic improvements of Charles's text to what he describes as censorship. Here King-Hele is referring to deletions of overly personal judgements, to family finances, including a reference to Erasmus's avowed desire to acquire wealth, and perhaps most significantly, an approving footnote comment on Erasmus's religious scepticism. After the publication of *The Origins,* this was a touchy subject in the Darwin household and mirrors the underlying ambiguity in Charles's intellectual relationship to his grandfather. It was Coleridge who, in 1796, described Erasmus as "everything, except the Christian," and the grandfather's materialism as exemplified in *Zoonomia* was stigmatised by Coleridge as the "State of Nature or the Orang Outang theology of the human race, substituted for the first chapters of the Book of Genesis."

In his observations on Erasmus, Charles seems torn between acknowledging the status and contribution of his forebear, and distancing himself from his methods and from his particular version of evolution. Erasmus, in the tradition of the eighteenth century, was a speculator, and Coleridge first coined the term 'darwinising' to describe his extravagant theorising. Charles was keen to record his opinion on this "overpowering tendency to theorise and generalise," but equally acknowledged that "his remarks . . . on the value of experiments and the use of hypotheses show that he had the true spirit of the philosopher." He had reason to be careful to draw distinctions since some of Erasmus's ideas, superficially at least, seemed close to those that Charles was most keen to claim as his own. For instance, on sexual selection Erasmus wrote: "The final cause of this contest among males seems to be, that the strongest and most active animal should propagate the species which should thus become improved." This is tantalisingly close to Charles's mechanism of natural selection, but in the *Autobiography* Charles maintained that although hearing evolutionary views early in life may have favoured him upholding them in a different form later on, he denied a direct influence. This apparent paradox can be explained by the emphasis that Charles gave to the importance of evidence to support a theory, in stark contrast, as he saw it, to the weight of speculation of those such as Erasmus compared to a meagre body of supporting observations. Charles's conclusion about Erasmus's evolutionary writings was that "his speculations on this subject cannot be held to have much value." Charles was perhaps justified in his fear of guilt by association since Bishop Wilberforce's in his highly critical review of *The Origins* had cast Charles as reviving the speculations of his "ingenious grandsire."

Publication of the English version of the Erasmus Darwin biography was to have unanticipated and unsavoury consequences. In May 1879, Samuel Butler, author, controversialist, and grandson of the Dr Butler who had been Charles's headmaster at Shrewsbury School, published a book entitled *Evolution Old and*

New in which he compared the evolutionary theory of Charles Darwin with earlier versions, including that of Erasmus Darwin. Although a supporter of the idea of evolution, Butler was critical of Darwin's mechanism of natural selection, and his book was motivated in part by a desire to reinvigorate support for these earlier versions that were based on a vital force in organisms to direct evolutionary change. Charles had a poor view of Butler's book, and indicated this in a note to Krause when he sent him a copy. Krause proceeded to incorporate critical remarks about Butler's work into his revised essay on Erasmus Darwin, and although Charles initially planned to mention that Krause's essay had been revised in his preface to the Erasmus Darwin biography, Krause did not want to have publicised the fact that his text had been abridged by Charles who consequently deleted reference from his proofs to the fact that Krause's essay had been revised.

The result was the clear impression that the criticisms of Butler were in the original German version of Krause's essay and that it had therefore been published prior to Butler's book. Butler was incensed, claiming that the Erasmus Darwin book was fraudulently antedated and intended as a covert attack upon him. He wrote to Charles on 2 January 1880, received an immediate explanation from him, but was not satisfied, and at the end of the month he wrote to the *Atheneum* thereby bringing the matter into the public arena. Although initially inclined to reply to the *Atheneum* explaining his position, Charles characteristically held council with family and friends. The advice was conflicting, and he decided not to respond publicly—a mistake, as his son Francis later privately acknowledged, as he was in the wrong and should have apologised. Instead he used his network of prominent establishment figures—notably Huxley—to respond to the attack, whereupon Butler, sensing a conspiracy, continued to make his accusations publicly through the press. Following Charles's secret encouragement, a translation of a letter of explanation by Krause was published in *Nature* in January 1881, but the matter was never settled to the satisfaction of either of them. Charles Darwin and Samuel Butler took a deep mutual antagonism to their graves, with Butler regarding Charles's public silence as an admission that he had no defence to make, and the Darwin family, as represented in *Life and Letters* edited by Francis, publicly content to see the issue "pass into a well-merited oblivion."

Those interested in the life of Erasmus Darwin, but more especially those interested in the totality of Charles Darwin's writings will particularly welcome this finely edited volume. In addition to a helpful introduction, Desmond King-Hele provides a synopsis comprising subject headings, which helps the reader navigate through an otherwise somewhat disjointed series of descriptions, observations, and interpretations that constitutes Charles Darwin's text. King-Hele provides very comprehensive notes, which give sources for Charles's quotations and brief biographical details of the individuals named in the text. In addition, these notes helpfully expand on the details of Erasmus's life. For those who want much more detail there is King-Hele's excellent biography published in 1999—*Erasmus Darwin: A Life of Unequalled Achievement.*

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THE DISCOVERY OF GLOBAL WARMING. *Spencer R. Weart. 2003. Harvard University Press, Harvard, MA. 228 pages. Hardcover, $24.95.*

This small book by Spencer Weart is a little gem. It should be required reading for scientists actively involved in climate change. Other scientists inter-

ested in global warming should first read this compact history of the subject before delving into the technical aspects in the science or popular literature. Most of all, it should be read by policy makers who increasingly will be called upon to make decisions and set policies regarding climate change. It is eminently readable and is suitable for people with little or no science background. Science jargon is largely omitted.

The book traces the history of climate-change science from its inception in 1896 when Arrhenius first published his calculations about the possibility of global warming from human emissions of carbon dioxide (CO_2). Though ignored by his peers, Arrhenius's bold statement was truly revolutionary for its time. Despite assumptions that were over simplified and rather naïve, the conclusions were remarkably accurate and extremely portentous considering what we know today. The first chapter covers the early studies and speculation concerning processes that might change the climate. It discusses the discovery that certain gases, notably CO_2 and water vapor, could "trap" heat and produce a "greenhouse effect." In 1938 a steam power engineer named Guy Callendar had the audacity to confront the Royal Meteorological Society in London with weather statistics he had collected as a hobby. To him they indicated the Earth was warming. He even said he knew what was causing it; the industrial burning of fossil fuels that emitted millions of tons of carbon dioxide gas into the atmosphere. Can you image that? What an audacious statement for a non-scientist to make. Unfortunately, it has taken over sixty years of research and heated arguments to prove him right. However, that's how science works. There are always a few farsighted individuals that happen on the truth, have it ignored, and then have it proved correct many years later—usually after they die.

Chapters 2 through 7 trace the early discoveries that global warming may be occurring, its possible causes, and the establishment of science groups to study the problem. Weart also discusses the great complexities of climate-change science and the difficulties of integrating the results of various disciplines. The study of climate change involves almost every science that deals with our atmosphere, hydrosphere, cryosphere, and biosphere. Often scientists in one rather narrow discipline of climate-change research are not aware of work in other areas that bear on their research. This can make progress slower than one would like. Numerous pioneers, such as Roger Revelle and Murray Mitchell, have contributed mightily to climate-change science. One of the more important discoveries was by Charles Keeling, who discovered that atmospheric CO_2 was increasing. He was instrumental in establishing stations to monitor the composition of the atmosphere. One was established on the slopes of Mauna Loa, Hawaii in 1958. I happened to be doing some fieldwork on Mauna Loa in 1964 when I visited the station. They showed me a six-year curve of steadily rising CO_2. I asked if they were going to publish it. At that time the duration of the rise was not well known (there were no ice core data), and I was told that it may just be a fluctuation that would return to normal after a few years. Forty years later it is still increasing. That curve is now known as the Keeling curve. Another disconcerting discovery that came from the analyses of ice-core data was that natural climate changes can occur in decades (perhaps less than ten years) rather than hundreds or thousands of years as previously thought. Could we trigger one of these catastrophic climate changes by warming the Earth? Weart discusses the growing evidence that human release of greenhouse gases by fossil-fuel burning and other activities was the primary cause of global warming, and how global warming was becoming a political issue.

Chapter 8 looks at the way new discoveries and much improved computer General Climate Models showed convincingly that global warming was indeed a fact and that humans were primarily the cause. This chapter is really not so much

a history as it is a summary of the current state of our understanding of global warming and the political attempts to do something about it. Weart cautions the reader that the chapter can only be considered a "preliminary sketch" of where we stand at present, without the long-term perspective of history. In 1988 the establishment of the Intergovernmental Panel on Climate Change (IPCC) went a long way to bring the problem of global warming to the attention of governments and stress the urgency of the problem. One of the results of the concerted effort of global climate change research was the Kyoto Conference of 1997 that attempted to set limits on greenhouse gas emissions. However, its implementation has been fraught with difficulty by some nation's, and particularly the fossil fuel industry's, attempt to downplay the problem. Weart leaves us with this thought, "Now the main question is what people will choose to do . . ." about global warming.

In Weart's final statement called "Reflections" he beautifully explains how science works in this most complicated of all subjects. Because of the sheer magnitude and complexity of the problem, the subject is fraught with uncertainties about the consequences of global warming and about various feedback mechanisms. However, almost everyone agrees that the consequences will not be good and may the catastrophic for humankind. Weart finally offers some ways to begin addressing the problems of global warming, and he warns that we need to begin now before it is too late.

In summary, this book is a magnificent attempt to document the series of events that led up to our current understanding of global warming. I strongly recommend it to the science and policy-making community. It is important to know how we got to this point in our knowledge of global climate change and to acknowledge some of the pioneers in the field who got us here. Spencer Weart should be one of the IPCC's participants in its next climate assessment.

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THE COMPANY I KEPT. *John Rodgers. 2001. Connecticut Academy of Arts and Sciences, New Haven, CT. 224 p. Hardcover, $35.00.*

This is a relaxed and informal account of John Rodgers's early training and education, later extensive travels, and continued and continuing education. Rodgers is a much, and justly, honored scientist and academic, well known for his contributions to structural geology and stratigraphy, as an educator and mentor, as coauthor of a famous text book, a distinguished editor, musician, linguist, and literateur, and so on—a many-sided man, yet with not a dilettante gene in his make up. Harold Nicolson, the English politician and diplomat of the inter-War years, remarked that he once knew a man who spoke sixteen languages fluently, but could say nothing sensible in any of them, including his own. There is nothing like that in Rodgers's achievements, for he clarified and illuminated all he touched.

It is almost a relief to find that the man (or his editor) is human and that there are some flaws in the text. For example, the Conklins are mentioned (p. 23) before we are properly introduced to them (pp. 94–97). In West Africa, Rodgers stood on a bornhardt and saw a gnamma without being aware of it. The late Brian Daily spelled his name thus; the Braggs (father and son) were awarded the Nobel Prize for their work on X-ray crystallography; and, if only out of kindness to the mere mortal, 'parrain' ought to be italicized to indicate that it is not an English word. I also question the preference for 'Englished' over 'anglicized'. Rodgers

also uses some interesting antiquated words, such as 'anent' and 'gait', the latter in a context that to me remains enigmatic. But these are minor idiosyncrasies in a wonderfully stimulating read.

Rodgers's reminiscences are what one would expect of the man: open, and full of interest. Besides introducing us to many geologists, we learn a good deal about the geology of fold mountains and the problems they pose, and also, and incidentally, about John Rodgers.

Rodgers was an indefatigable but purposeful traveler. (I wish I had his frequent traveler points). He met many colleagues most of whom became friends. While not a *Who's Who of Twentieth-Century Geology*—this is a realm peopled by Yale and East-Coast geologists and their contacts (though there are some omissions), so that West-Coast luminaries, for example, are relatively neglected—it is nonetheless an important and influential world. For me one of the delights was in finding personal details about the people behind work I have admired—Eleanora Bliss Knopf (her 1924 GSAB paper that gives an early clue to unequal erosion); T. N. Dale (granitic structures in New England); Scheibner (his synthesis of NSW geology); McConnell (pseudotectonics in Ghana); Gèze (boulders of magmatic origin); Rubey (many seminal papers); and so on. It is also fascinating to witness, bit by bit, the accumulation of the principles on which Rodgers has lived and worked. All can be summarized as tolerance; which, however, is not to be confused with weakness, for while keeping an open mind, Rodgers has always been rigorous and judicious in his treatment of ideas.

But above all this book is about people who have had the good fortune to meet and come to know Rodgers. Indeed he has a gift for friendship. With few exceptions, those he met are treated with warmth and generosity. At one point in his reminiscences, when he had chosen to join the faculty at Yale, he remarks that he had arrived where he belonged. It was the great, good fortune of Yale and of generations of students and colleagues that he did so. John Rodgers is not only a Renaissance man par excellence, but above all, a fine human being. We owe a debt of gratitude to the Connecticut Academy of Arts and Sciences for making available to all this inspiring record of a life well-lived.

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JOSEPH BANKS. A LIFE. *Patrick O'Brian. 1987. The University of Chicago Press, Chicago. 328 p. Softcover, $14.95.*

Like many independently wealthy persons through the ages, Joseph Banks used his advantages wisely and to the general good. Even now we remain in his debt. Though best known for his botanical work on James Cook's first voyage of exploration on the *Endeavour*—and rightly so, for from it flowed the *Floriglegium* and eventually Kew Gardens and much else—, Banks became a scientific statesmen and long-time president of the Royal Society, benefactor to many gifted in the sciences and arts, and a public servant in the widest and best sense. He served and made important contributions to many groups and committees, ranging from the boards of Agriculture and of Longitude, and the African and Linnaean societies, to committees dealing with the draining of the Fens and the dispensation of local justice. His role in the establishment of a European settlement in New South Wales, and hence Australia, is well known. He encouraged and directed the Kew plant collectors, who journeyed to out-of-the-way and difficult places in

search of specimens. He was instrumental in introducing merino sheep to England. And so on—his was a multifaceted career.

Banks had a gift of friendship at all levels of society from royalty to his estate staff. He received many honors, both national and international. He was a great Englishman and a fine human being. He achieved all this in the course of a long life (77 years), despite being incapacitated in his later years by obesity, gout, and associated afflictions.

Banks was an inveterate letter writer. Some have claimed him as one of the best letter writers in English, a claim with which one can agree so far as substance is concerned, but which is manifestly false as to style. His material is invariably interesting, but his punctuation and grammar disturbing. And one wonders whether O'Brian has not subconsciously modeled his writing on that of his subject. For in the account under review the fascinating story of Banks's life is swamped by details and digressions, some of the latter not obviously germane. The text is replete with convoluted sentences. Dependent clause follows dependent clause. Ands, buts, colons, and semicolons abound. Some sentences embrace 150 words or more. The author is obviously erudite, but he neglects his readers. In his running heads, O'Brian gives the year under review followed by, e.g., AET 27. To one raised in the soccer religion aet denotes 'after extra time', but is in this context an abbreviation for the Latin *aetatis* which means 'at the age of'. The meaning of "osp," which appears in the genealogical table (p. 16), was unknown to the reviewer and may be to other readers unversed in genealogical shorthand. It is short for *obiit sine prole,* or 'died without issue'. O'Brian refers condescendingly to "that very great man Cook" (p. 234). Some of the language is ugly or inaccurate. For example he refers (p. 201) to "the house they *pitched* on"—why not selected, or chosen? He refers to Banks travelling down to Revesby (p. 204) from London—why down? Is the Fenland village noticeably lower than London? Some statements are misleading. On p. 168, for instance, O'Brian states that Banks was splendidly equipped to write the *Florilegium,* yet he dwells at length in his Preface on the shortcomings of Banks's writing; and his unsuitability for the great task was apparently corroborated by Banks himself, as cited on p. 171.

O'Brian had access to sources not available to previous biographers, and his account covers every aspect of a complex life. Disappointingly, in its presentation the book does less than justice to its subject.

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INTERESTING PUBLICATIONS

Gerald M. Friedman, CONTRIBUTING EDITOR

Since the start of this journal, Founding Editor Gerald M. Friedman has prepared this column. Contributors wishing to list recent books and papers of interest to our membership are requested to send them to Professor Gerald M. Friedman, Brooklyn College and Graduate Center of the City University of New York c/o Northeastern Science Foundation, Inc., Rensselaer Center of Applied Geology, P.O. Box 746, Troy, NY 12181-0746 U.S.A.; gmfriedman@juno.com

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Efgenji E. Milanovsky. Subsequent to the events described in these reminiscences, Professor Milanovsky (b. 1923) became head of geology at Moscow State University, and a Member of the Russian Academy of Science. In 2001, he was awarded the Academy's A. P. Karpinsky Gold Medal for his series of fundamental work on problems of regional geology of Russia and adjacent countries, tectonic structures, the development of continents, orogenesis, and riftogenesis. This prestigious medal is only awarded once every five years. His special research area has been the Caucasus region, where he worked on neo-tectonics and neo-volcanism, and accomplished extensive mapping (up to the summit of Elbrus Volcano at an altitude of 5642 meters), also proving the occurrence of Pliocene glaciation in the region. This work yielded a 1500-page monograph. He is the author of over seven hundred papers, and about thirty books (some co-authored), including his massive three-volume *Geology of the USSR.* He has also been actively involved in the study of the history of geology and is a strong supporter of the work of the International Commission on the History of Geological Sciences, regularly providing his deft sketches (drawn on the run during field excursions) for the Commission's Newsletters. Besides being very well informed on art history, Professor Milanovsky has published a biography of Wegener (in Russian) and is just completing a history of the two hundred years of the Geology School

at Moscow University. For a further account of his early life, including his role in a tank unit in the Russian Army, all the way from Moscow to Germany, see: "Interview with Professor Eugeni Milanovsky, Freiberg, 22 September, 1999," *INHIGEO Newsletter,* No. 32, 2000 for 1999 (Sydney), 30–33.

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